

CONCLUSION

What Kind(s) of Ethnography Does Political Science Need?

EDWARD SCHATZ

The best protective device against being taken in by one particular language is to be brought up bilingually or trilingually.

—Paul Feyerabend

When Feyerabend wrote about protecting oneself against being "taken in" by one particular language, he sounded a concern that resonates in the contributions to this volume.¹ No field of social inquiry should be metaphorically monolingual; to allow a pragmatic and intellectually open exchange between people working in different traditions, researchers should be conversant in several research "languages." Of course, to be bilingual or trilingual is not to abandon structures of grammar and representation that make for coherence. One who speaks both Spanish and English is able to communicate more ably and widely than one who speaks only "Spanglish."

In this conclusion, I address a series of issues related to a core question: What kind(s) of ethnography should be "spoken" in political science? After suggesting a way to think about the value of political ethnographic approaches offered by this volume's contributors, I ask what is distinctive about ethnography as it is used to study politics. In other words, why do we deploy the phrase "political ethnography"? I then consider two kinds of political ethnography and what their promotion would entail in the current intellectual and professional climate in political science. The first is a kind of ethnography that is used in multiple-methods research projects. The second is a kind of ethnography that stands alone. A third possibility—that nothing short of a hegemony of political ethnographic methods and sensibilities would suffice—is something that I leave to the polemicists uninterested in the kinds of pluralism that Feyerabend sought to create.

Overall, I suggest that stand-alone ethnography is crucial. I attempt to make the *hard* case that *even* neopositivist, causality-oriented theorists intent on generating decontextualized knowledge have good reason to protect space for stand-alone political ethnography. Moreover, they should embrace *even* that kind of political ethnography that distances itself from the project of producing causal theories. Because of the variety of sometimes conflicting ideas about ethnography and its value (many of which are covered in the preceding chapters), I think of the arguments in this conclusion as contributing to an ongoing discussion about, rather than as an attempt to offer the "final word" on, political ethnography.

When Political Ethnography?

Under what conditions and given what research questions is ethnography useful?²² Could we identify specific, real-world conditions that recommend ethnography? Lorraine Bayard de Volo and I once contended that ethnography is particularly valuable *inter alia* "in cases where government statistics are suspect, media outlets are controlled by political interests, and poverty, lack of infrastructure, illiteracy, or political violence impede survey research" (Bayard de Volo and Schatz 2004, 269). While ethnography may be the only viable technique in some circumstances, it does not logically follow that in other circumstances ethnography is less useful. To the contrary, as the preceding chapters make clear, ethnography is crucial across a wide range of research circumstances. Walsh's chapter makes the point elegantly: even in the advanced industrial United States, with its abundance of government statistics, armies of pollsters, impressive media freedom, and relative infrequency of political violence, participant observation is crucial to our understanding of how public opinion is formed.

Putting research *conditions* to the side, do some research *problems* inherently lend themselves to ethnographic inquiry? Could it be that various aspects of micro-politics—quiet forms of deliberation, resistance, and mobilization; enactments of power relations that occur in the "private" domain; novel and contextual recombinations of social and political factors that generate unanticipated dynamics, and so on—to a degree *require* ethnographic approaches?

Following Shapiro (2005), I am convinced that "problem-driven research" is preferable to its "methods-driven" alternative. Yet the call for problem-driven research implies that "problems" are timeless abstractions. Are they? I view them as situated in particular flows of knowledge in particular disciplines at particular times. Or, if one prefers a less fluid meta-

phor, "problems" are embedded phenomena—embedded in the bedrock of disciplinary knowledge formed from the accumulated sediment of many-layered debates and practices, both inside and outside the academy.

Take a seemingly timeless "problem"—the relationship between banks and the state in advanced industrial economies. In 2004 one might reasonably have studied this relationship using quantitative measures of financial flows. By late 2008, when an intricate web of financial networks and dubious lending practices had become exposed, an ethnographic approach to reveal the interrelationships among key actors would have been invaluable.³ In the abstract, the "problem" (banks and the state) had not changed, yet fundamentally different aspects of this relationship seemed to "deserve" study in 2004 than in 2008. The crucial point is this: methods—whether ethnography or others—do not flow seamlessly from particular questions taken as timeless abstractions (Schatz 2007).

Whether ethnography is recommended depends, I argue, less on the topic being studied and more on how the topic has been studied to date. That is, if ethnography is suited to uncover processes, discourses, and behaviors that generate a political dynamic that is important but scarcely visible (Scott 1990), then what is invisible depends on where our spotlight shines. Like the proverbial drunk who searches in vain for his keys only in the light of the streetlamp, we must search beyond what is currently illuminated.

The issue is principally epistemological. If micro-politics are hidden, it is largely because analysts have not (yet) gotten near enough to their inner dynamics.

Why Political Ethnography? Stepping Away from Holism

Why use the word *political* to describe what we do? Imagine a book on "political statistics" and the strangeness of the modifier becomes apparent. We emphasize that our work focuses on *politics*, because ethnography (unmodified and unmarked) has become so closely associated with the study of culture and society.

Our project represents a distancing from the attempts to represent *holistically* a culture, society, group, or locale that earlier generations of ethnographers offered.⁴ Consistent with the postmodern condition, we do not claim complete knowledge or aspire to a full representation of social reality. Recognizing that, as Verdery (1996, 204) puts it, "What we can understand of something depends on how we think our way into it in the first place," we know that our accounts are delimited by our *a priori* assumptions. Our

analytics are therefore partial; we study politics—the exercise of power in its varying manifestations.

Still, political ethnography is also about ethnography, and the ethnographer does not seek prematurely to hive off the political realm before learning where its boundaries might reasonably be viewed to lie.⁵ Few political ethnographers pretend to holism, but we attend to concatenations of contextual factors more than nonethnographers do. In other words, we typically map the part (politics) in relation to the whole (the sociopolitical context, both in its temporal and locational sense).⁶ And we are likely to view a wider range of objects—sorcery, coffee-shop talk, slaughterhouse work, waitress uniforms, rural road signs, home repair problems—as inherently, and sometimes deeply, political than would most nonethnographers. We happily scour the seemingly less political realm for evidence of power relations at play.

Taken together, the two parts of the term *political ethnography* thus imply a creative tension. *Ethnography* suggests a particularizing impulse, a desire to avoid premature empirical generalization, and a preference for inductive thinking. It implies attention to detail, to contextual factors, and to configurational thinking.⁷ *Political* suggests a willingness to bracket aspects of what we see, to simplify for analytic coherence, and to seek to produce generalizations. It implies attention to cross-case comparisons, to broadly occurring factors, and to the power of deductive logic. The phrase thus contains the potential for each impulse to perform a check on the other, in the process producing empirically grounded and theoretically stimulating research.

Broadening how we understand the political realm is crucial to what a political ethnographer does. This does not, however, mean that everything is equally power-laden or that the manifestations of power relations are everywhere of equal importance. When an ethnographer studies politics, he is examining something vertical, often unspoken, and, therefore, potentially hard to reach. Inevitably, questions of access emerge. Can an ethnographer gain access to the truly powerful actors of "high politics"?

This is a particularly thorny question in the subfield of international relations (IR), where—the argument often goes—the level of analysis encumbers attempts at ethnographic inquiry. It is true that the constructivist IR scholarship that emerged in the 1990s proceeded from a core ethnographic sensibility—from a desire to probe the plural "inner logics" that govern the behavior of states (thus problematizing the commonplace assumption that states are homogeneous and essentially similar units). But is

IR limited by its core, field-defining research problems, such that ethnography as participant observation is unlikely?

The question is a valid one: are many of the interesting topics about the exercise of power simply beyond the reach of the participant observer? The answer has to be yes, but this yes should be qualified. First, many of the access problems that participant observers face have little to do with whether one studies "high" or "low" politics. Conducting ethnographic immersion about matrimonial relations between spouses might be no more possible than conducting a similar study about the causes of war. Challenges of scale and secrecy are real, but they are not confined to high politics. Moreover, the requirements for access can be formidable even in the most mundane affairs; think about the linguistic and cultural fluency, as well as the trust of interlocutors, that is the *sine qua non* of access for any participant observation. If one lets relative accessibility dictate the terms of research engagement, many fundamental questions about politics will go unaddressed.

It would be folly to deny that the National Security Council of the United States, for example, is unlikely to welcome a participant observer in its ranks; to expect access—whatever one's background characteristics, skills, and trust gained from interlocutors—would in this case be naïve. But access is a sliding scale, not a binary. The remote village, hermetically sealed from the outside world, no longer exists (and probably never did, except in the early ethnographic imagination); it is no longer useful (if it ever really was) to view the ethnographer as either "in" the village or "outside" it. The political ethnographer strives for the *nearest possible vantage point* to study a given problem. Interviews that fall short of participant observation, or participant observation in a site that falls short of the ideal location, are the stuff of real-world research compromises. Nonetheless, the political ethnographer will make every effort to achieve proximity and intimacy as a route to knowledge. To the extent that she falls short, she provides reasons why.

The principle of striving for the nearest possible vantage point indicates new possibilities for social-scientific work. Indeed, cultural anthropologists have begun to move beyond fetishizing the ethnographic method to embrace the study of increasingly complex global flows. As Comaroff and Comaroff (2003, 156–57) reflect:

It is only by broadening our frames of reference that we may address some of the awkward questions that have come to confront us about our methodology: can we be sure, for example, that "the particular" we seek to study or

the cultural worlds we presume to exist, may actually be *empirically* bounded? Is "the local" not the constantly refashioned *product* of forces well beyond itself (Appadurai, 1996; also 1997)? Does it not exist only as part of a socio-political geography of multiple scales and coordinates (Ortner, 1997)? Is it not true that the singularity of places, just like the singularity of "traditions," "customs," and "cultures," is being fashioned ever more in response to the market? Surely, neat antinomies between the local and the global, between field and context, between ethnography and metanarrative, beg the very questions that we should be asking.

Whatever difficulties exist for the ethnographer to access high politics, we have examples of successful ethnographic work in IR. Séverine Autesserre (2006) conducted ethnographic interviews (Spradley 1979) with "peacebuilders" in the eastern Democratic Republic of Congo to see how they understood violence. Michael Barnett (2003) reflected on his ground-level experience at the U.S. mission to the United Nations to understand how powerful actors could permit genocide to develop in Rwanda in 1994. Catherine Weaver (2008) used insider knowledge of the World Bank to argue that organizational culture weighs heavily on what the bank does. Though neither uses participant observation, Vincent Pouliot (2007b) employs a Bourdieu-inspired ethnographic sensibility to chart how diplomacy itself helps to constitute a security community,⁸ and Ted Hopf (2002) deploys a core ethnographic sensibility to redescribe the contours of Russian foreign policy.

In short, when we consider these problems of access, we should weigh the extent to which they emerge from the topics being studied, on the one hand, and, on the other, the extent to which how a subfield imagines itself places artificial constraints on its possibilities. Admitting that access problems are real should not serve as an invitation to scholars to abjure the proximity that is the hallmark of ethnographic work.

What Kind(s) of Political Ethnography?

Should every student of politics become in part a political ethnographer? Metaphors that liken methods to "tools" in a "toolbox" (Bayard de Volo and Schatz 2004; Anderson 2005) imply that a capable researcher—who would be equally trained in Bayesian statistics, participant observation, interview techniques, discourse analysis, and so on—would simply reach for the tool appropriate for her current research problem. And, while some simple household chores could be completed with the aid of a single im-

plement, most common tasks would require the use of a variety of tools. Ethnography, in this view, is most useful when combined with other tools of inquiry.

With proponents of the toolbox metaphor, I agree that ethnographic approaches may be combined usefully with other methods, but multiple-methods research contains limits. Consider first some practical limits. Becoming trained in and employing any method takes time and involves sunk costs. The typical political ethnographer must learn the complexities of a context—the language, the cultural assumptions, and the history—before beginning method research. While nonethnographers may also learn some of these complexities, the ethnographer faces particularly significant time commitments that preclude or at least postpone the development of other analytic skills. The trade-offs continue with fieldwork. The researcher usually invests months, if not years, developing relationships of trust with interlocutors and refining his knowledge of the research context. Given these practical realities, most scholars build entire careers on their graduate school training.

Other limits are epistemological. To develop research skills is also to create sunk intellectual costs; put differently, research styles are habit-forming. Few scholars move effortlessly between the rigors of, for example, postmodernism and those of neopositivist social science; the logics, styles of inquiry, requirements, attributed meanings, and rewards vary too widely. The claims to knowledge embedded in ethnography can be reconciled easily with some methods, less easily with others.

What Happens to Ethnography in Multiple-Methods Projects

Multiple-methods work by Ashutosh Varshney (2002) illustrates the challenges of combining ethnographic and nonethnographic methods.

Like David Laitin (1998), Varshney offers a serious attempt to measure the power of multiple-methods approaches. Varshney begins with a question: What produces interethnic peace in some Indian cities, while in others Indian cities spiral into interethnic violence? Arguing that an understanding of micro-level is required to tease out an answer, he uses an array of techniques (principally interviews, though some statistical measures are used) and his methods to test an explanation for some ethnic violence in a city endemic in Aligarh, Hyderabad, and Ahmedabad, and in a city with no ethnic violence in Calicut, Lucknow, and Secunderabad. He concludes that ethnic violence and civic engagement are likely to maintain a balance in the first three cities, but other types are less likely to do so. In the last city, civic engagement is the only

like "ethnicity" and "civic engagement," this argument easily "travels" to contexts far beyond the subcontinent—something that makes it a first-rate book in comparative politics.

This research design has rightfully been lauded for its willingness to attend to micro-political factors (King 2004), but it is worth asking what effect the design has on his research conclusions. On one hand, the *targeted* use of ground-level techniques helps Varshney to distinguish between superficially similar cases and explain outcomes that otherwise would be paradoxical. It helps him to accomplish a "process tracing" that links his independent variables (types of civic engagement) and dependent variables (ethnic peace or violence). Without this, there would be an enormous empirical gap.

On the other hand, several things occur to the subjects under study with such a targeted use of ground-level techniques.¹² First, the study's focus begins narrow and remains narrow; he is interested in what drives the production of interethnic violence or the maintenance of interethnic peace. He proceeds by first constructing an event-history dataset of violence in India to pinpoint which cases to use for ground-level study. Then he examines the secondary literature (archival records, newspaper and other periodical accounts) about these cases. Finally, he conducts fieldwork that involves human subjects (elite interviews, semi-structured interviews with a sample of ordinary people). In no way is Varshney insensitive to the tragedy that is ethnic conflict, but once he has contact with his research subjects, they are essentially useful as founts of information to test his variables.¹³ Ground-level techniques here are in the service of a narrowly defined theoretical agenda; they are not used to pursue knowledge inductively. And even if induction plays an actual role, presenting one's work as deductive has come to be the preferred rhetoric.¹⁴

Second, the people being studied—when they appear—do so in a "flattened" way. Varshney claims that his sampling technique for interviews—using illiteracy to guide the sample—allows him to "hear the voices of the subaltern" (2002, 20). But, while these voices may have informed Varshney's background thinking, they are scarcely heard. Oral histories (as opposed to more structured interviews) are not cited individually (though they are cited as a group [344n10, 352n5]), and when evidence from interviews with ordinary people is used, what makes it to print are the responses to quantifiable (that is, closed-ended) questions (for example, 126–27).

Nothing is wrong with this; Varshney has made reasonable choices.¹⁵ His goal—to contribute to decontextualized knowledge about ethnic conflict as a route to its prevention—simply runs up against the goal of letting

individuals speak.¹⁶ Indeed, Varshney contrasts his task with that of the ethnographer: "Having been trained as [a social scientist], I thought my comparative advantage lay in social science, not in constructing ethnographies or writing fiction" (xiv).¹⁷

In this sort of multiple-methods research, insider meanings cannot be given adequate expression. To attend to insider meanings, Varshney would have had to do at least some of the following: (1) let his subjects identify what being a Muslim or Hindu means, what violence and peace mean, and what civic engagement means—rather than positing their significance *a priori*; (2) let his subjects identify reasons for ethnic conflict and mine these reasons for valid information; and (3) let his subjects identify the conditions under which ethnic conflict is a salient topic and what related social facts his interlocutors associate with the topic. While the outsider with a research agenda is understandably determined to elicit responses on a particular subject, ordinary people typically have a complex array of associations and considerations.¹⁸ A purer ethnographic account would give pride of place to interlocutors' worldviews, but when ethnography is subsumed by a very different logic, such an account is not possible.

Well-executed research such as Varshney's adds empirical flesh to theoretical relationships and offers opportunities to perform "process tracing" about empirical situations. But the limits are also clear: larger epistemological-ontological commitments subsume ethnography. Typically, such commitments reduce ethnography to ground-level data-gathering techniques that hold little space for insider meanings and concatenations of contextual factors.¹⁹ A disciplinary atmosphere with an overriding commitment to neopositivist causal theory building refracts ethnography in decidedly nonethnographic directions—unless that ethnography stands alone.

Stand-Alone, Extrinsic-Value Political Ethnography

Imagine a continuum of stand-alone, political ethnographic work.²⁰ At one end is political ethnography whose *raison d'être* is external: it exists to produce evidence for decontextualized knowledge.²¹ To the extent that ethnographic material gathered contributes to discussions based on empirical generalizations, it is considered valuable; whatever does not contribute to such debates is considered extraneous. This is *extrinsic-value* political ethnography.

Whatever potential the extrinsic-value political ethnographer has for contributing to decontextualized knowledge, her contribution may be underappreciated for several reasons. Consider what would happen, for

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Well-executed research such as Varshney's adds empirical flesh to theoretical relationships and offers opportunities to perform "process tracing" about empirical situations. But the limits are also clear: larger epistemological-ontological commitments subsume ethnography. Typically, such commitments reduce ethnography to ground-level data-gathering techniques that hold little space for insider meanings and concatenations of contextual factors.¹⁹ A disciplinary atmosphere with an overriding commitment to neopositivist causal theory building refracts ethnography in decidedly nonethnographic directions—unless that ethnography stands alone.

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example, if she offered an account that was in essence *consistent* with existing, decontextualized accounts. In the standard neopositivist narrative of science of cumulative knowledge, her study would be welcomed as increasing confidence of a theory's validity—a successful “test” of that theory. But the narrative and the reality diverge. In real-world professional contexts, such a study would often be dismissed as “merely another case of something we already knew” from the general theoretical account. If only for this practical reason, political ethnography is better positioned to problematize existing decontextualized accounts than to offer support for them.

If a political ethnographer offers an account *inconsistent* with existing decontextualized theory, her work might serve as a corrective. Consider Allina-Pisano (2004). Based on ground-level familiarity with agricultural reform in post-Soviet Ukraine, fluency in the region's languages, and carefully cultivated trust of her interlocutors, Allina-Pisano demonstrates that local authorities in the 1990s resisted neoliberal economic reforms for reasons that the theoretical literature overlooks. They sought not to protect their “rents” but rather to preserve Soviet-era rural institutions, which “served as socioeconomic anchors for their communities, providing employment, social services, inputs for household production, and food for urban populations” (557). The implications for the entire postcommunist region are important: to be successful, market-oriented reforms must pay attention to both local leaders and ordinary people. This study posits a different way of thinking about the value-rationality of postcommunist elites. That they are self-serving, even corrupt, individuals is one possibility; Allina-Pisano's work suggests an alternative.

Such political ethnography keeps those bent on abstract, decontextualized theorizing in check, forcing them to consider alternative renderings of empirical reality (Shapiro 2004), asking them to interrogate assumed causal relationships, and raising the possibility that what passes for knowledge can be based on specious conclusions. Such work potentially generates new directions for decontextualized research.

Unfortunately, however, extrinsic-value ethnography is unlikely to be more than a corrective; practical professional realities militate against it. Unless the political ethnography can be constructed as a “critical case study” (Eckstein 1975), findings that are anomalous are too often dismissed as “outliers.” Indeed, the deck is stacked *against* ethnographically generated, cross-contextual, empirical generalization, since such generalization would require that entire teams of ethnographers conduct a coordinated, simultaneous project on a variety of contexts. How many trained political eth-

nographers are available and willing to conduct a study that would apply Allina-Pisano's insights to Moldova and Kazakhstan, let alone to Mauritania and Cuba? And if their findings confirmed the validity of her insights, would the value of any single political ethnographic account be adequately recognized?²²

If a study like Allina-Pisano's generates enough intellectual traction and attention—and this depends on any number of professional and intellectual trends that would take us far afield—it could serve as a seminal idea for additional research. But again, to the extent that data for such research projects can *only* be produced via intensive research (her findings, after all, depend on proximity to local elites that is sufficient to discern their intentions, something that does not come easily), the deck is stacked against broad, decontextualized theorizing based on ethnographically generated insights. Moreover, the difference between a seminal and a still-born idea too seldom emerges from the idea's inherent qualities.

If the potential, extrinsic-value political ethnographer is thinking strategically, he might choose a different research direction. Recognizing that an empirically grounded, confirmatory study is undervalued, he might shift his approach at the outset to increase the number of "cases."²³ The growing number of cases in turn increases the chances that his study would be published and recognized, whether it confirms or questions existing theoretical accounts. At the same time, he drifts from political ethnography, since more cases compromise his ability to conduct immersion. Like Varshney, he might opt for a targeted use of ethnography, even if the potential for ethnographically generated insights remains unrealized.

Stand-Alone, Intrinsic-Value Political Ethnography

At the other end of the continuum is political ethnography whose *raison d'être* is internal. Its value inheres in the research process itself. By *intrinsic-value political ethnography*, I do not have in mind work that completely lacks connections to issues outside the empirical context from which it is drawn; whether or not it is desirable, no such work is possible. I am convinced that scholarship necessarily treats normative concerns, deploys analytic concepts, and employs empirical reference-points that are *tout court* nonindigenous.

Stand-alone political ethnography maximizes its potential when relieved of the chore of doing work it cannot reasonably be expected to do. It cannot and should not aspire to establish broad empirical generalizations. It cannot and should not seek to establish causality, understood as

the probability that a decontextualized factor X leads to decontextualized factor Y. It cannot and should not purport to generate data that can be easily and meaningfully converted into quantities that might be subjected to statistical tests.

Relieved of these tasks, political ethnography can do much more. Instead of aiming to offer empirical generalizations, establish probabilistic causal relationships, or contribute a data-point for statistical analysis, it can produce conceptual innovation. Flyvbjerg (2001) adroitly shows that in the natural sciences, space for such conceptual innovation is crucial. Imagine what would occur if this space went unprotected in the social sciences. If Scott (1985) had been preoccupied with generalizing the empirical extent, frequency, and intensity of hidden forms of resistance—such as foot-dragging, gossip, and sabotage—he would have had to deploy a series of nonethnographic tools that likely would have hampered his ability to discover these forms of resistance in the first place. If Walsh (2004) had developed an overriding concern with finding a “representative” group of U.S. citizens for ethnographic immersion, we would know much less about how political opinions are formed. If Wedeen (1999) had set out statistically to “test” the causal impact of official discourses in authoritarian contexts, we would have been deprived of her crucial insights about the relationship of ordinary people to the state discourse in Hāfiz al-Asad’s Syria. I strongly suspect that quite a few potentially valuable political ethnographies are never written because researchers feel motivated to pursue other objectives.

Political ethnographers are, of course, correct to reflect seriously on issues of empirical scope and causality. “How common is what I have identified?” and “How compelling is the evidence for the causal claims I am making?” should be natural questions for any scholar. But if a scholar *begins* inquiry with these questions, he or she is likely to focus only on those aspects of social or political life whose salience makes them easily visible and whose clarity makes them easy to trace. Political ethnographic work reminds us that the most visible and clear factors are not necessarily the most important ones.

Students of politics need not share the motivations of the intrinsic-value political ethnographer to see the importance of promoting such work, *even if it is disengaged from the preoccupying logic of neopositivist causal social science*. We know that insider viewpoints are a crucial part of human communities and social and political processes, but we also know that the central preoccupation with causal theorizing makes it difficult to consider them. As Wood (2007, 125–26) puts it:

An important category of data unavailable except through observation or face-to-face interaction with subjects is the preferences and beliefs of political actors as varied as union members (Lipset 1956), local elites (Dahl 1963), parliamentarians (Fenno 1978), and lobbyists and activists (Graetz and Shapiro 2005). How actors understand their identities and interests is often best approached through interviews, observation, and surveys designed specifically for that purpose. General public opinion surveys may be useful for some questions but often do not ask the right questions or do not ask them such that the results are relevant for scholarly analysis. In particular, how political actors perceive their strategic interactions with other actors in real settings—what choices they confronted, their beliefs concerning the likely consequences of different choices, their analysis of paths not taken—is often unavailable except through face-to-face interaction with the actors themselves, that is, through field research.

Let me put it more crudely: insider meanings and complex contextuality cannot be plugged into a regression equation, so one must either marginalize them (the current disciplinary solution) or create a space for research that attends to them (a more productive row to hoe, in my view). When intrinsic-value work flourishes, it provides a rich starting point for those interested in empirical generalization. Ultimately, it is far richer than the thinned empirical accounts that are encouraged when political ethnographers are asked to orient themselves to extrinsic goals.

Beyond conceptual innovation, the value of intrinsic-value political ethnography may simply be that to conduct ethnographic research on human beings is to grant legitimacy to their predicaments and concerns. It is typically to bypass top-down, often state-driven imperatives. To publish ethnographic work, in turn, is potentially to let the people being studied "speak," an exercise that gives voice to the powerless, the subaltern, and the understudied; it is therefore an inversion of the usual relationship between researcher and researched.²⁴ In turn, letting the subject speak may have implications for policy. When welfare recipients, victims of shantytown violence, or women who have lost children in civil wars are heard, their predicaments are brought to light, where they otherwise would remain in shadow.²⁵ The value of such research thus inheres in a combination of normative commitments and empirical focus.

All of this implies a need to advance intrinsic-value ethnography. Ethnography should not be *the single* goal of political science research, but it should be *among the goals* of political science research.²⁶ I imagine a professional political science that "speaks" a variety of ethnographic (not to

mention nonethnographic) "dialects"—both intrinsic-value and extrinsic-value. This is a call for an ethnographic shift at the discipline-wide level, as a way to counter what Schram (2004, 417) calls a "scientistic drift." This shift would not reduce ethnography to ground-level techniques of data-gathering. Rather, it would centrally consider insider meanings and complex contextuality—and I repeat—even if this meant distancing the project from questions of causality.

The risk, of course, is that stand-alone, intrinsic-value ethnography could be marginalized by those in the discipline who categorically deny its potential value. The risk is real, and it implies responsibilities. Political ethnographers face the responsibility to make their research both intelligible and relevant to nonethnographers. They need not assume neopositivist logic or adopt its vocabulary, but they should address abiding questions about political and social life whose relevance is recognizable.

At the same time, nonethnographers face the responsibility to read widely and to consider ways of using stand-alone ethnographic research to inform their own (nonethnographic) studies of politics. Moreover, they must recognize that allowing space for ethnographic work in the institutional settings of universities demands, for example, that Institutional Review Boards (IRBs) sensitize their reviewers to the unusual requirements and challenges of ethnographic work.²⁷ Simultaneously, nonethnographers must resist the temptation to view stand-alone ethnography as "merely" exploratory research, as "simple" hypothesis-generation, and therefore as somehow inferior. Thus, any ethnographic shift will require a broad shift in sensibility across the discipline.

Political ethnographers and nonethnographers must find ways to build bridges across an eclectic and broad political science (Sil and Katzenstein 2005; Yanow, this volume). Substantive and methodological specialization is desirable (Morrow 2003), but only if the discipline simultaneously encourages much boundary-crossing (Drezner 2006) or, at a bare minimum, a recognition of the artificiality of boundaries.²⁸ I sense that students of politics are up to the task.

Notes

1. Feyerabend (1979, 91), as quoted in Deising (1991, 50).
2. Thanks to an anonymous reviewer for stimulating this line of thought.
3. For an insightful ethnographic account of Wall Street investment bankers, see Ho (2005).
4. Most of cultural and social anthropology has also abandoned attempts to holism, as Kubik details in chapter 1.

5. Kubik in chapter 1 describes neoclassical economists as having performed a "hiving off" by isolating the formal aspects of the economy assumed to be universal and ignoring the ways in which economic systems are themselves deeply embedded in specific cultural logics. See also Day (2005).
6. Thanks to a reviewer for encouraging this phrasing.
7. On configurational thinking, see Ragin (2000).
8. The essence of Pouliot's argument thus recalls Wedeen's emphasis on "performative practices" offered in chapter 3.
9. David Laitin is among the exceptions, having changed both geographic (from Africa to the former Soviet Union) and methodological (from ethnography and narrative to greater emphasis on game theory and statistics) focus. See *Qualitative Methods* (2006).
10. Thanks to Varshney (private communication) for his comments on Schatz (2007), which inform this discussion. For a similar argument about Laitin's work, see Hopfl (2006).
11. *Ethnic* is Varshney's term to distinguish Hindus from Muslims in India.
12. My own work (Schatz 2004) likewise suffers from at least some of the shortcomings that I identify here.
13. Varshney might respond that testing such variables helps to tease out explanations that, ultimately, could guide conflict-prevention programs in the future.
14. In a spirited defense of his work, Laitin (2006, 31–32) contends that the central insight for his 1998 book—namely, that a new identity of "Russian-speakers" was emerging in ex-Soviet states—came directly from (inductive) fieldwork. If this is the case, then it is a puzzle why the book is couched in the rhetoric of *a priori* deductive theorizing. Laitin (private communication) has pointed out that all social science involves both deduction and induction. This is surely true, but it says nothing about the role, sequencing, or proportions of inductive versus deductive thinking. On these latter issues, see Pouliot (2007a).
15. See Varshney (2006) on basic trade-offs involved in the design and execution of any research project.
16. While Varshney proposes to steer a middle course between "stylized facts" and "thick description" (2002, 21), his is a causal story about independent and dependent variables, rather than a narrative about particular people in particular places at particular times. Whatever its theoretical contribution, Varshney's account holds little room for the human beings who dot the analytic landscape.
17. The implication that ethnography is opposed to, rather than a type of, social science is one that should be questioned.
18. When I described Varshney's book to my (educated, nonspecialist) neighbors in Toronto who are originally from Calicut, they insisted that most people rarely think about ethnic violence and peace: they are more concerned with getting their next meal. The contours of an ethnography need not be dictated by the interlocutors' point of reference, but they ought to carve out space for it.
19. Flyvbjerg (2001) argues that predictive theories are impossible in the social sciences not because of their inability to capture "subjective" factors, but because of their inability to attend to context, which is essential to the human experience.
20. By "stand-alone," I mean ethnography that is published separately from nonethnography. Most political ethnography is conducted in the gray area between the ideal types I identify here.
21. One way to understand ethnography's position is to consider Aristotle's distinction

- between *phronesis* (contextualized knowledge) and *episteme* (abstract knowledge). Most ethnographers would agree with Flyvbjerg (2001) that while both *phronesis* and *episteme* are valuable, social science's great strength lies in the former rather than the latter. That is, they find much value in producing contextualized rather than decontextualized knowledge. For a debate on the applicability of Flyvbjerg's distinction to political science, see Schram (2004) and Laitin (2003).
22. The political ethnographer himself might be skeptical of the validity of cross-contextual empirical generalizations, given what he knows about his unique in-field conversations, the role of in-field contingencies, and an improvisational (in Yanow's sense from chapter 13) approach to research. Even ethnographic accounts of the same geographic context can vary widely. See Heider (1988).
 23. Ragin and Becker (1992) usefully draw attention to the variety of ways of thinking about what constitutes a "case."
 24. This is, of course, one possibility among many. Ethnographers and ethnographic knowledge were part and parcel of the European colonial enterprise (Said 1979) and the development of the Soviet state (Hirsch 2005). To this day, states employ ethnographers to "discover" (often construct) traditions that serve to legitimate an elite's authority (Schatz 2004). Large corporations now hire researchers who use ethnographic techniques to learn the behavior of ordinary people—as a way to improve marketing appeals. (Thanks to Dorian Warren and Dvora Yanow for revealing this interesting development on the Interpretation and Methods Listserv.) As Bayard de Volo mentions in chapter 10, the U.S. military has also begun to employ ethnographers.
 25. On welfare recipients, see Soss (2000). On shantytown violence in Brazil, see Arias (2004). On mothers who lost children in Nicaragua, see Bayard de Volo (2001). The intrinsic-value ethnographer must convince those without an inherent interest in the subject that his subject deserves attention. Even if one studies violence or poverty—issues with a normative dimension that should speak for itself—it may still not be clear why a study of violence or poverty of one group deserves more attention than a similar study of another group.
 26. It may be appropriate for some research to put insider meanings into the background. Most research in political economy, for example, would likely eschew such considerations, even given central insights of work that is fundamentally meaning-oriented (Herrera 2006b; Mwangi 2002). See Herrera (2006a) for a brief argument similar to what I present here.
 27. For an exploration of the role that IRBs play in political science, see the special issue of *PS: Political Science & Politics* (41 [2008]), especially Yanow and Schwartz-Shea (2008).
 28. Thanks to Patrick Thaddeus Jackson (personal communication) for this latter point.

WORKS CITED

- Abbès Kinois. 1992. "Bâtir la nation, une tâche pour tous les chrétiens." CEDAF, Brussels. Dossier sur les événements récents au Zaïre (janvier-février 1992). 2358 III 1992. 5 February.
- Abbott, Andrew. 2001a. *Chaos of Disciplines*. Chicago: University of Chicago Press.
- . 2001b. *Time Matters: On Theory and Method*. Chicago: University of Chicago Press.
- Abélès, Marc. 1992. *La vie quotidienne au Parlement européen*. Paris: Hachette.
- . 1997. "Political Anthropology: New Challenges, New Aims." Blackwell/UNESCO. September.
- . 2004. "Identity and Borders: An Anthropological Approach to EU Institutions." Twenty-First Century Papers: On-Line Working Papers from the Center for 21st Century Studies, University of Wisconsin-Milwaukee.
- Abu-Lughod, Lila. 1986. *Veiled Sentiments: Honor and Poetry in a Bedouin Society*. Berkeley: University of California Press.
- . 1990. "The Romance of Resistance: Tracing Transformations of Power through Bedouin Women." *American Ethnologist* 17, no. 1:41-55.
- . 1999. "Comment on 'Writing for Culture.'" *Current Anthropology* 40:13-15. Special supplement on "Culture—A Second Chance?"
- Ackelsberg, Martha A. 1991. *Free Women of Spain: Anarchism and the Struggle for the Emancipation of Women*. Bloomington: Indiana University Press.
- Adler, Emanuel. 2002. "Constructivism and International Relations." In *Handbook of International Relations*, ed. W. Carlsnaes, T. Risse, and B. A. Simmons. 95-118. London: Sage.
- Adler, Glenn. 1992. "The Politics of Research during a Liberation Struggle: Interviewing Black Workers in South Africa." In *International Annual of Oral History*. 1990: *Subjectivity and Multiculturalism in Oral History*, ed. Ronald J. Grele, 229-45. New York: Greenwood Press.
- Agar, Michael H. 1980. *The Professional Stranger*. New York: Academic Press.
- Aguilar, J. 1981. "On Anthropology 'at Home.'" In *Anthropologists at Home in North America*, ed. D. Messerschmidt, 15-28. Cambridge: Cambridge University Press.
- Ahnen, Ronald E. 2007. "The Politics of Violence in Democratic Brazil." *Latin American Politics and Society* 49, no. 1:141-64.
- Alliance for Workers' Rights. 1999. "Sexploitation in Nevada's Casinos." April newsletter.